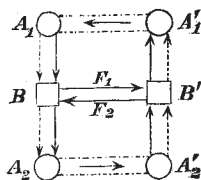


heights. It is thus quite conceivable that, in a scale of space foreign to our graphic measures, the free roving ærialian set may all occupy a common place in this foreign scale of space, and that a massed and a moored ærialian point may have the same position in graphic space without impinging on each other, as such points are not at the time at one and the same place in the foreign space. The moored or bound ether may thus traverse the space occupied by the massed ether of gross matter without mutual interference; but, whether superposed or not in ordinary space, the pair of ether sets which compensate resultant actions of two gross-clustered sets of a pair of points of baric matter, will form, however they may mingle graphically, two orbs of ether exerting each (corporately) exactly counter-equal free-orb couples.

If for example the baric points B and B' are urged towards each other with a ponderomotive force or flow of ordinary baric momentum F_1 , by the motor actions on them of two ether-orbs $A_1 A_1'$ in counter equal intensities, force-momentum only will be



transferred from A_1 to A_1' ; while the tractive momentum (or as we may presume, the heat-energy) accompanying it will only be transferred from A_1 to B , and a similar transfer of thermal energy or tractive-momentum will at the same time take place from B' to A_1' .

Should it, in the next place, be required to oppose the action F_1 by an equal counter-force F_2 , a pair of ether-orbs $A_2 A_2'$ must be superadded to those already urging B and B' , so as to urge them in the opposite direction. It will be seen from the figure that the total effect of this and of the previous orb-pair's actions will simply be that the pair of ether-orbs connected with B will transmit motor energy from one to the other (from A_1 to A_2), and the other ether-pair will also transfer an equal amount of energy contrariwise from one orb to the other, without any leakage of ordinary momentum occurring at B and B' , by the neutralised action, into the channel $B B'$.

Newcastle-on-Tyne, February 10 A. S. HERSCHEL

(To be continued.)

Terrestrial Radiation and Prof. Tyndall's Observations

IN NATURE, vol. xxvii. p. 377, I see a notice on Prof. Tyndall's observations on terrestrial radiation, with the author's concluding remarks, that meteorologists should not be offended by his saying that from outsiders equipped with the necessary physical knowledge they may expect valuable aid towards introducing order and causality among their observations. May I be permitted to state that Prof. Tyndall will give no offence, at least to the meteorologists whose works are advancing this science at the present time.

Prof. Tyndall tries to prove by his observations the extreme importance of vapour of water as a check to terrestrial radiation, and he mentions the much greater difference between a thermometer in the air four feet from the ground and another on cotton wool on a morning when snow was lying on the ground than on other nights, equally clear, but with higher temperatures of the air and no snow. Now it is well known that, *pari passu*, a surface of snow will be colder than a surface without, because (1) snow is an excellent radiator; (2) because, as a very bad conductor, it shelters the surface from the influence of the higher temperature of the soil. In the observation on December 10, the thermometer on cotton wool was so cold because it was under the influence of the cold radiated by the snow, and besides immersed, so to say, in the coldest stratum of air near the ground. To my mind, the manner in which the observation was conducted does not prove what Prof. Tyndall advances. To isolate, so to say, the influence on radiation of the atmosphere itself, he should have placed, between two poles, at some feet above the ground, a plank, and on it his cotton wool and thermometer. No doubt that this thermometer, isolated from the snow, should have shown a higher temperature than his thermometer placed on the surface on cotton wool.

Prof. Tyndall lays great stress on the fact that the difference between the temperature in the air and on the ground was less in clear nights with a higher temperature and greater quantity of vapour of water in the air, and sees in this a confirmation of his opinion on the great influence of vapour of water in checking radiation. I do also see in this the influence of vapour of water, but not of its absolute quantity, but of relative humidity. Once the dew-point is attained, the cooling of the thermometer on the ground is arrested. The whole question between Prof. Tyndall and many physicists and meteorologists is this: nobody negates the influence of vapour of water on terrestrial radiation, but Prof. Tyndall ascribes this influence to vapour in the gaseous state, while his opponents hold the opinion that in this state vapour of water has a diathermacy scarcely different from dry air, while, condensed in small ice crystals or water droplets, it really interposes a very efficient screen to terrestrial radiation, even if, which sometimes is the case, it is perfectly transparent to light, *i.e.* invisible to our eye. Another influence of water on terrestrial radiation is admitted by all: that is, that of the latent heat in the deposition of dew and hoar frost.

If we wish to make meteorological observations bearing on the question, the following *modus operandi* should be adopted: (1) observations should be made in climates where, with a tension of vapour greater than that which obtains in England in winter, the relative humidity is yet so small that there is no dew on clear nights, or at least it appears rather late; (2) three thermometers placed on cotton wool, but at different heights above the ground should be observed, say one on the ground, and the others at heights, say from 10–100 feet above.

If Prof. Tyndall's views are right, the highest of the thermometers should show by far the lowest temperature, as it is not screened from radiation by the vapour of water diffused in the lowest stratum of air. I think every meteorologist will express the opinion that there will be scarcely a difference in this case. As to the observations in different climates, those made where the relative humidity is low should give no greater difference between the thermometer in the air and on the wool than the observations in England on clear nights, with the same vapour tension, if Prof. Tyndall's hypothesis be admitted. I think we have already many observations which prove that, with vapour-tensions much above $0^{\circ}181$ (or 4.6 mm.), *i.e.* above that of saturation at 32° F., terrestrial radiation is very great, if only the sky is clear and the relative humidity small. No doubt the decrease of the temperature of the air from the midday maximum to the night minimum is caused by terrestrial radiation. I give some figures from the observations at Biskra, in the Algerian Sahara.¹

	Difference of daily max. and min.	Mean temperature.	Tension of vapour.	Relative humidity.	Amount of cloud.
January ...	25.4	56.8	0.264	61	1.6
August ...	39.2	89.6	0.557	40	0.8
October ...	35.6	68.4	0.432	58	0.9

In an arid climate in low latitude the non-periodic variations are but small, and the difference between the maxima and minima is very near to the daily range of temperature. As the amount of cloud is very small in all three months taken here, the conditions for terrestrial radiation are very favourable. If vapour of water in the gaseous state impeded terrestrial radiation so much as stated by Prof. Tyndall, we should expect to find the daily range smaller in August than in January, on account of the double amount of vapour in the air. The reverse is the case, the daily range being by 14.8 greater in August than in January. Has anybody observed a daily range of 39.2 in England, be the amount of cloud and the vapour-tension ever so small?

I must add that in all observations bearing on terrestrial radiation we must not forget that other substances besides water in its three states may interpose a screen to radiation. I mean especially dust and smoke of all kinds. Now far from large cities, there are many reasons why in winter, especially when the ground is covered with snow, the air will hold less of these impurities than in summer, as in winter there are no fires of forests and peat-bogs, there is little inorganic dust, because the humidity of the soil, and still more so the snow, prevent it; organic dust, germines, &c., are also absent, or present in very small quantities, on account of the small amount of plant and lower animal life. The absence of dust and smoke explains the great purity of the air in winter, so favourable to solar and terrestrial radiation.

¹ "Annales du Bureau Central Météorologique de France," 1879, vol. ii.

tion, as well as the purity of the air at great heights, especially above the snow-line.

Prof. Tyndall has certainly lost sight of this when he attributes the diathermacy of the air in winter only to the small amount of vapour of water. The same is the case when he points to the relatively small nocturnal radiation on clear nights in many tropical countries. In the case of many of them, besides dust and smoke, the *high relative humidity* has much to do with the small amount of cooling during the night. What quantities of latent heat are liberated by the formation of dew in humid climates of low latitudes, and how much the nocturnal cooling must be impeded by it, everybody can imagine who has been in these countries, or only read scientific travels to them.

A. WOELKOF

Diurnal Variation in the Velocity of the Wind

THE observations discussed in Mr. Buchan's interesting article on this subject leave little to be desired, and with most of the conclusions meteorologists in general will agree. I am surprised, however, to find such an eminent authority accounting for the large diurnal oscillation on land, solely on the ground of its being due "to the superheating of the surface of the ground, and to the ascensional movement of the air consequent thereon, which tend to reduce the effects of friction and viscosity of the air."

There may perhaps be more hidden within this sentence than appears from the wording of it; but, taking it as it stands, it certainly omits what I believe to be the most important factor in the whole result, viz., the *interchange of motion between the upper and lower layers of the atmosphere, occasioned by the ascensional movements during the day over superheated land*. This has been most clearly shown by Dr. Köppen in an article in the *Austrian Zeitschrift für Meteorologie*,¹ by successive rejection of inefficient causes, to be the only means by which such increase of velocity could be occasioned near the earth's surface.

It is not clear, moreover, how the ascension currents could otherwise diminish the friction of the air enough to account for such a large diurnal increase of velocity. The effect of the increased temperature alone, would certainly be to increase the friction, but as Köppen shows from Meyer's formula for the coefficient of gaseous friction, the daily range of temperature would only cause the friction of the air to vary from $\frac{1}{2}$ to 1 per cent. of its whole amount,² so that this factor is evidently without any appreciable influence on the diurnal period.

In the paper already referred to, Dr. Köppen has gone into the whole question most minutely, and a perusal of it will, I think, convince most persons, that the chief factor in causing the diurnal increase of wind-velocity over land is the intermixture of air (*luft-austausch*) resulting from the uprise of heated air from the surface, and the consequent downfall of cooled air to it, "bringing down with it," as Espy told the British Association in 1840, "the motion which it has above, and which is known to be greater than that which the air has in contact with the asperities of the earth's surface."

Among the facts cited by Köppen in favour of his theory may be noted the following:—

1. The fact that in Europe the ratio of the velocity of the wind to the gradient, is greater for N.E. winds and in summer than for S.W. winds and in winter; together with the circumstance that the temperature decrement, and therefore also the facility with which local ascension and descension currents may be formed, is greater under the former conditions than under the latter.

2. That simultaneously with the diurnal increase in the velocity of the lower layers of the atmosphere, those above appear to be retarded.

3. That on stations near the earth's surface the curve of absolute humidity reaches its minimum about the time of maximum wind-velocity, while at elevated stations, such as the Faulhorn, the humidity reaches its maximum at the same time.

In fact it may be concluded, as Köppen graphically puts it, "that the greater the difference of the temperature of the air in a vertical direction, the smaller are the differences in the humidity, barometric pressure, and motion of the air, and that in the early hours of the afternoon the inhabitants of plains are placed to a certain extent on a higher, and the dwellers of Alpine heights on a lower, level, relatively to these elements."

E. DOUGLAS ARCHIBALD

¹ "Die tägliche periode der Geschwindigkeit und Richtung des Windes," September heft, 1879.

² Meyer's formula in English measure is $\eta = \eta_0 (1 + .0014\theta)$, where η , η_0 are the friction coefficients at θ and 32° Fahr. respectively.

The Large Meteor of March 2, 1883

THE meteor described by Mr. R. W. S. Griffith in the last number of NATURE was also observed at Bristol and Bath. At the latter place it was seen by Mr. J. L. Stothert at 9h. 33m. 40s., passing in the direction from α Hydræ to η Canis Majoris. The brilliancy of the meteor was equal to twice that of Venus; colour yellow; motion slow; no train. Comparing this observation with that obtained by Mr. Griffith, it would seem that the meteor probably belonged to a radiant point near Lyra, rising in the north-north-east at the time of its appearance. A meteor shower was observed by the writer on March 14, 1877, between 14h. 12m. and 15h. 43m. from the point α 277, δ 25°, the members of which were somewhat slow and devoid of streaks or trains, and the fireball of March 2 last appears to have belonged to the same stream.

It would be important to hear of additional observations of this meteor. Its considerable brightness, and the fact that it appeared at a time when it must have been widely observed, lead me to hope that many other records of its path have been preserved. In all such cases it is very desirable to give the R.A. and Dec. + or - of the beginning- and end-points of the observed path. Descriptions by the stars or compass-bearings are likely to be less accurate, and are often difficult to reduce.

In the *Observatory* for September, 1879, p. 129, I mentioned that "during the first four days of March fireballs have been very numerous, especially on the 1st, 2nd, and 4th." This meteoric epoch is therefore well confirmed by the fireball of the 2nd inst. which it is hoped will aid us in determining one of the chief radiant points of the date.

W. F. DENNING

Bristol, March 12

A VERY brilliant meteor was seen here on March 2 at 9.35 p.m. It burst forth in the immediate neighbourhood of Sirius, and passed downwards to the west at about an angle of 40° from the perpendicular, disappearing after a course of about 25° . Its light was so strong as to make the distant trees, fields, and hedges perfectly visible, brighter than the brightest moonlight. Its colours also were very decided, changing quickly, much as does Sirius to the naked eye, but showing more of the violet at first, and afterwards more of the red.

J. L. J.

Capel, Surrey

On the Movements of Air in Fissures and the Barometer

I SHOULD be glad to add to my article "On the Movements of Air in Fissures and the Barometer" (NATURE, vol. xxvii. p. 375) a reference to an instrument devised by Mr. Whitehouse, and described in 1871 before the Royal Society (*Proc. Roy. Soc.* vol. xix. p. 491). The apparatus, which was intended to record minute variations of atmospheric pressure, consisted of two hydraulic chambers, connected by a tube or siphon, and buried in the ground. One of the chambers was left open at the top and exposed to atmospheric pressure, the other was closed and removed from such pressure; the difference in the level of the water in the two was a measure of the variation in the atmospheric pressure. This instrument reproduces those conditions to which the oscillation of the water-level in certain chalk-wells, coincident with the barometric changes, has been attributed. It was believed by the inventor that by its aid he had been able to detect atmospheric waves or pulsations at a distance from a storm-centre; it has not however come into scientific use.

I may further add to my brief allusion to colliery explosions a reference to the paper by R. H. Scott, M.A., F.R.S., and W. Galloway, Mining Engineer, entitled "On the Connection between Explosions in Collieries and Weather" (*Proc. Roy. Soc.* vol. xx. p. 292, 1872).

A. STRAHAN

28, Jermyn Street, March 10

THE PITT-RIVERS COLLECTION

IT will be remembered that some time past Major-General Pitt-Rivers, F.R.S., most munificently offered his far-famed Anthropological Collection to the University of Oxford on the condition that the University should erect a building adequate to contain it and display it properly. On Wednesday, the 7th ult., a vote was passed by Convocation authorising the Curators of the University